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visual process which was based upon *general* properties of the nervous substance would be open to the same objection. This is true, and it applies to Müller's own explanation of the phenomenon in question with peculiar force. But the conclusion to be drawn is not that one visual theory is sure to be just as good as another, but rather that that theory which posits a chemical process which is *not* exactly like what goes on everywhere else in the body has by so much the advantage over another theory. The idea of a photo-chemical substance which is unstable after a partial dissociation, which it is as far as possible from being a remote idea to the chemist, is *just as far* removed from our conception of other physiological processes as it *must be*, in a well-devised theory, in order to account for anything so extremely distinctive as is the visual after-image.

But even though it had been necessary to go very far afield for the conception of a semi-suicidal chemical substance, this could not have been counted, by any one who had given a moment's consideration to the subject, as a point of superiority on the part of Hering's theory over mine; for his assumed photo-chemical substance is 'suicidal' from the start. If blue is the color of assimilation, then after we have looked at a blue surface for a few moments there has been piled up in the retina, according to Hering, a large amount of the blue-yellow substance, and it is the going to pieces of this immediately afterwards which is the cause of the after-image; this assumed process is not in itself an objection to the theory, but it is 'suicidal' to the last degree.

Professor Müller's recent papers in the *Zeitschrift für Psychologie* are a monument of learning and acumen, as I have already said in the pages of *The Psychological Review*. How far they are from substituting for the original theory of Hering a theory which can lay any claim whatever to being considered an adequate account of the phenomena of color-vision I am about to show in connection with a general discussion of color theories. Meantime I rejoice in the fact that Professor Titchener has renewed his study of the subject of color. It is to be hoped that this will lead him to remodel the brief statements regarding color which are found

in his book on Psychology; what he says there (while it is not incomprehensible to one who has the clue to his secret meaning) must seem contradictory and confusing in the extreme to the ordinary reader, and certainly constitutes a serious blemish in a book which is otherwise not simply a good text-book, but a valuable contribution to the science of psychology.

C. LADD FRANKLIN.

BALTIMORE.

#### A PRECISE CRITERION OF SPECIES.

TO THE EDITOR OF SCIENCE: I thank you for the suggestions contained in your kind discussion in SCIENCE, No. 178, of Mr. Blankinship's and my paper on a 'Precise Criterion of Species.' Our paper was concerned with a method which, if applied, will constitute a small, but, we think, important, step toward giving greater precision to the defining of particular species and to the distinguishing of varieties from species. To my mind the only important objection urged so far, an objection which was anticipated, is that it is impracticable to use in systematic work so great precision as our method calls for; it takes too much time and too large a number of individuals. *A priori* argumentation cannot dispose of this formidable objection; only the demonstrated advantage of the method in practice can avail against it. I should like to urge anthropologists, mammalogists, ornithologists, ichthyologists, malacologists and others who have already gone some way in the direction of applying statistics to species to put the method to practical test. Mr. Blankinship and I are doing so. I should be very glad to assist those who meet with difficulties in the application of the method, as, for example, in the measurement of color and complex forms. The ingenious naturalist will find, however, as anthropologists have found, few, if any, specific differentiae which are not measurable.

C. B. DAVENPORT.

#### ELECTRICAL ANÆSTHESIA.

TO THE EDITOR OF SCIENCE: While making some experiments on the sensations derived from sinusoidal currents I noticed (April 12, 1898) that anæsthesia of the tissues resulted